How Has U+2 Enforcement Affected Housing Affordability in Fort Collins?

Using the Synthetic Control Method to Analyze

The Affordability Implications of Residential Occupancy Regulation

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Table of Contents

Abstract

In 2007, the City of Fort Collins began enforcing “U+2”, a residential occupancy restriction created in the 1960s which limited the number of unrelated persons who may cohabit legally. In a 2005 study, Corona Research estimated that 1,070 households were in violation of the ordinance. This study employs the synthetic control method (SCM) (Abadie and Gardeazabal 2003; Abadie et al 2010) to estimate the effect of enforcing U+2 on the cost of housing in Fort Collins, Colorado. The SCM is a data-driven comparative case study methodology which allows the researcher to estimate the impact of an intervention (such as U+2) on an outcome variable – in this case, the cost of housing. The results of this SCM are sensitive, but they indicate consistently that U+2 enforcement materially increased the cost of housing in Fort Collins following the beginning of its enforcement in 2007.

Introduction

In this study, I seek to understand the impact that enforcement of an occupancy restriction ordinance in Fort Collins, Colorado – called "U+2" or the "Three Unrelated Persons" ordinance – has made on residential affordability in the city. I refer to the ordinance primarily as "U+2". To avoid over-occupancy citations, landlords and tenants must follow the ordinance, which states: "Occupancy in a residential dwelling unit (single-family, duplex, and multifamily) is restricted to: one family … and not more than one additional person; or, one adult and their dependents (if any), a second adult and their dependents (if any), and not more than one more additional person" (City of Fort Collins Land Use Code 3.8.16).[1] The ordinance has existed since 1965, when it was applied to "ensure health and safety of residents, and to help protect the quality and character of neighborhoods" (City of Fort Collins 2018), but it was almost never enforced until 2007, when, at the behest of disgruntled homeowners, the city began implementing U+2 through a resident complaint-based system (Corona Research 2009, 14).

This study contributes to the body of work that investigates the effect of regulation on housing supply and affordability. Though not the only investigation of how U+2 impacts the rental housing market in Fort Collins (Corona Research 2005; Corona Research 2009; Corona Research forthcoming),[2] it is unique in the methods employed and the question investigated. As

[1] "Family" is defined as "any number of persons who are all related by blood, marriage, adoption, guardianship or other duly authorized custodial relationship, and who live together as a single housekeeping unit and share common living, sleeping, cooking, and eating facilities" (City of Fort Collins Land Use Code 2018, 5.1.2).

[2] Corona Research, Inc. is an independent group that has been retained by the City of Fort Collins to study the effects of enforcing U+2. The group produced an original study in 2005, an updated study in 2009, and is working to complete a retrospective study, which similarly assesses the effect of enforcing U+2 in the city. The 2005 study focused specifically on the impact on the rental market (price, vacancy). The 2009 study, conducted after the city began enforcing U+2, examined the impact of enforcement on different groups of the city's residents. I do not have further information about the current update, which should be completed in 2019.

a result, this research provides a more sophisticated methodological insight to the impact of U+2 enforcement on affordability than other extant studies. In this study, I attempt to answer the following question: How has the city's enforcement of U+2 affected residential affordability through the City of Fort Collins?

Investigations of the impact to housing affordability rendered by artificial restriction of housing supply through regulation tend to be methodologically problematic. Despite an influx of research studying the regulation of housing supply, much remains unknown about its precise causes and effects. Further, while my project contributes to literature on the interaction of housing regulation and affordability, a cross-sectional study alone would not provide as neat an analysis as panel data. Gyourko and Molloy (2014) note that in addition to surveys which enable panel data, "researchers should explore other readily-available data that might shed light on the degree of regulation," and that it might also be fruitful to design creative instruments for regulation and its potential determinants, so that causal estimation can be performed without panel data (63). In an attempt to circumvent problems associated with data availability, budget and time restraints, and, in particular, the lack of a control against which Fort Collins can be compared, I perform the synthetic control method (Abadie and Gardeazabal 2003; Abadie et al 2010) to estimate the effects of U+2 enforcement on rental affordability in the city.

The synthetic control estimates average home values that Fort Collins would have experienced were the occupancy ordinance never enforced. To simulate the trajectory of rent prices without the enforcement of U+2, the method creates a synthetic city based on weighted characteristics from cities with similar economic and demographic profiles, and housing

markets to Fort Collins. Change of home values in the synthetic city over the studied period is then compared to actual, extant data for change in home values in Fort Collins. This enables an estimation of the degree to which housing costs deviate from a less regulated trajectory previous to U+2 enforcement (McClelland and Gault 2017).

I use home value data retrieved from Zillow, called the Zillow Home Value Index (ZHVI). With limited access to resources related to housing and rental prices in Fort Collins, it provides the best data I can access.

To construct my synthetic control, I extracted from Corona Research (2005) a list of 16 cities of similar size to Fort Collins which experienced growth patterns similar to what was predicted for Fort Collins between 2005-2015. Similarity of size and growth pattern is identified by total household growth, household growth among traditional college-age students, and a growth rate higher in the second group than the first (Corona 2005, 14). In the ratio of growth between college-age households and total household growth, Provo, Utah and Sioux Falls, South Dakota were the closest to Fort Collins. The former is a college town and the latter, not. However, Sioux Falls hosts a few small (student body less than 2,500) liberal arts schools and a for profit university. These cities are pulled from a list of comparable locations in Corona Research (2005, 14).

I anticipate that following the enforcement of U+2, the cost of housing in the City of Fort Collins will have risen in a manner it would not have in the absence of U+2. A general rise in rent prices would account for increased demand of rental units following the enforced vacation of bedrooms that otherwise might have been occupied by renters, especially those college-aged. More pointedly, it is reasonable to anticipate an increase in prices of residential units

smaller than the relatively-large, single- family home which housed most violators of U+2 (Corona Research 2005).

Background

Fort Collins is a home-rule city and the seat of Larimer County, Colorado. Rapidly growing, it is presently the largest municipality in its county: the 2017 American Community Survey (ACS) estimate of the city's population is 165,080, nearly half of the county population, 343,976 (United States Census Bureau 2017). Fort Collins dominates Larimer County's employment base, and this is not expected to change although the county's projected growth rate outpaces the city's in coming years. The county is expected to reach a population of 400,000 by 2030 and 600,000 by 2060; however, Fort Collins' share of the county population is expected to decrease during that time (City of Fort Collins 2011). Development of the city's built environment is governed by a growth management area (GMA), which was established in 1980 (Larimer County and Fort Collins 2008).

The cost of living in Fort Collins has risen rapidly since 2000. In Larimer County, between 2001 and 2018, the cost of basic needs – housing, child care, food, health care, transportation, and miscellaneous items, as well as the cost of taxes and the impact of tax credits – grew by 62 percent, significantly outpacing the growth of the median wage, which increased by only 40 percent during the same period (Pearce 2018). Among basic needs in Larimer County, the cost of housing rose at the second-highest rate, 57 percent, after the cost of childcare. This proportionate growth in housing cost is equal to the state average during the same period (Pearce 2018, 13-14). In Fort Collins, however, increase in the cost of housing outpaces that of Larimer County. Between December 2008 and 2018, the Zillow Home-Value Index (ZHVI)

increased for the city by 73 percent, 4 percent higher than the increase of the same measure in the county (Zillow 2019; Zillow 2019a). I investigate if, and to what degree, the rise in housing costs after 2007 is attributable to the residential occupancy restriction.

History of U+2

U+2, defined in the City of Fort Collins Land Use Code (3.8.16, 2018), states that for any type of dwelling unit "the maximum occupancy allowed per dwelling unit … shall be either" one family as defined in the Land Use Code (2018, 5.1.2) and not more than one additional person; or two adults and their dependents; or one adult and their dependents, another adult and their dependents, and not more than one additional person. This means that if more than three people occupy a dwelling unit, only one of those people can be unrelated to the others. If more than three unrelated cohabiters occupy a dwelling unit, then those individuals are in violation of the ordinance and subject to a penalty up to $1,000 per person, per day of violation following a one-month period provided by the city to find suitable housing.

A few variances to the occupancy ordinance exist, though they are not assumed to materially affect this study. The first variance is a Host Family Permit, which allows owner-occupant families of single-family dwellings to host an additional resident for a ten-month period, pending approval by the city and compliance with several restrictions. Host Family Permit participation is not expected to materially affect the housing market. Another variance allows qualified properties to become Extra Occupancy Rental Houses (EO). Few zones within the city allow EO houses, and an EO property is "a building or portion of which is used to accommodate, for compensation, four or more tenants, boarders, or roomers" (City of Fort

Collins 2018). In 2016, only 42 single-family properties and nine Colorado State University affiliated properties had received permits for EO (Douglas 2016).

Fort Collins has used occupancy restriction for over half a century. Occupancy restriction was written into the Fort Collins municipal code in the 1960s and it has never left, though the nature of the restriction and its enforcement have evolved substantially in the last decade (City of Fort Collins Neighborhood Services 2018). Once coded a criminal offense, the ordinance was modified, becoming a civil offense punishable by fine before the city began actively enforcing in 2007.

In its present form, the ordinance is highly controversial, and the city has commissioned three studies on its effects. Prior to intensifying enforcement of U+2, the city retained a private research firm to perform an economic impact analysis, which predicted the immediate and long-term impacts of U+2 enforcement (Corona Insights 2005). Later, in 2009, Corona was retained to perform a supplementary, follow-up study of the effects of U+2 enforcement. Presently, Corona Insights is conducting a third, retrospective study of the ordinance's effects (forthcoming). This third study is partially funded by the Colorado State University student government and university administration, with each group contributing $10,000 to the study's estimated $77,000 total cost (Duggan 2018). Proponents of the ordinance believe it to preserve and enhance neighborhood character, bolster school quality, and improve quality of life in the city (Walker 2014). Opponents cite that it is discriminatory – historically, toward agricultural workers, and in its current practice, toward students – and that the ordinance contributes to the city's heightening affordability woes (Fowler 2014; Douglas 2016; Coltrain 2018).

Corona Insights conducted the city's first study of U+2 in 2005, to predict the impact enforcement. The research firm performed an economic and market study, which profiles the population of U+2 violators and estimates immediate and long-term impacts of enforcement. The study estimates that in 2005, 1,070 households were in violation of the ordinance. It predicts that over 5,000 renters would be affected by enforcement of the ordinance, by it either causing them to move or downsize their household – removing residents from the home (Corona Insights 2005, 2). Using a public survey, Corona determined that residents living in close proximity to violator households are significantly more likely to identify neighbors hosting disruptive parties and noise, parking issues, and other problems; additionally, neighbors of U+2 violators were more likely to express negative perceptions of their neighborhood's character and criminal activity (Corona Insights 2005a, 4; Corona Insights 2005, 2).

The research firm's profile of the population in violation of U+2 reveals the current problematics behind U+2 enforcement. The study provides insights to households in violation of U+2, which reveal that, most often, violator households were inhabited by college-aged (but not necessarily college-attending) individuals. According to the study, 82 percent of tenants in households violating U+2 were aged under 25, while 71 percent were college students (Corona Insights 2005, 4). While the study finds that the average sum incomes of violating households was higher than those of non-violating renters, individual tenants of violating households tended to be lower income and experienced poverty at a rate of 52 percent (Corona Insights 2005, 3). Provided this information, it is clear that enforcement of U+2 targets college students and residents of similar age, a vulnerable population which suffers from food and housing security issues at disproportionate rates nationwide (Silva et al 2015).

Furthermore, the study insinuates that early enforcement of U+2 would introduce a shock to the structure of demand in the city's rental market. According to the study, 64 percent of households in violation of U+2 resided in single-family homes, while a significant portion of them occupied units with more than three bedrooms (Corona Insights 2005, 3). Therefore, many households found in violation of U+2 would be forced to split and downsize, potentially leaving bedrooms uninhabited. Accounting for this assumption, Corona estimated how households would reform to satisfy the ordinance and determined that the majority of renters relocating in response to strong U+2 enforcement would create two-person households with lower combined incomes, placing a significant increase in demand for rental units in the $550 to $775 price range (Corona Insights 2005, 5; Corona Insights 2005a, 47). It is expected that strong enforcement of U+2, beginning in 2007, resulted in some increase in the price of rentals between $550 and $775, monthly, before enforcement, and for other residential units suitable to low-income, two-person households.

In Corona Insights' follow-up study, which investigates the effects of U+2 enforcement on the city's economy and housing market, the market's response is partially explained, though the impact of U+2 enforcement is neither quantified nor conclusively addressed otherwise. Between 2005 and 2009, the number of households in violation of U+2 is estimated to have dropped from 1,070 to 579 (46 percent), though only 20 percent of that drop is due to active enforcement (Corona Insights 2009, 2). The report notes that rental prices had increased between enforcement and the follow-up study, though an explanation for the phenomenon is merely gestured at. About rental prices, the report notes, "Rental prices are increasing, but not disproportionately to other comparable Colorado cities. It is likely that this is due to the fact

that vacancy rates were high when the ordinance was initially enforced" (Corona Insights 2009, 2). Therefore, to develop a more robust understanding of the consequences of U+2 enforcement, its impact on housing affordability should be further investigated.

Literature Review

U+2 is a regulation that limits the supply of housing potentially suitable for cost-sharing, cohabiting renters and owners. Though most of the estimated 1,070 violating households in 2005 were occupied by college-aged renters, an estimated 168 owner-occupied households stood in violation of the ordinance as well (Corona Insights 2005, 55), indicating that there was a substantial population of violators composed of various "extended households". Extended households take several forms, being composed of people related by blood or law, people entirely unrelated, or comprising some mix of the two (Koebel and Murray 1999, 125-126). Extended households are most commonly composed of multi-generational families but are frequently constituted by a group of unrelated cohabiters which would violate U+2. By precluding the legal assembly of households larger than three unrelated persons, U+2 limits the quantity and type of dwelling unit that cost-sharing renters can occupy with impunity, therefore restricting the supply of housing suitable for this group. While the literature of local finance, urban economics, and housing studies reports with apparent ubiquity that increasing regulation negatively affects affordability (Gyourko and Molloy 2014), potential cohabiters' option value forbidden by way of U+2 enforcement suggests that renting households of greater size would display greater willingness to pay for a home rental when that cost could be split among a larger group of people (4 or more unrelated persons). Where research suggests that U+2 enforcement should have reduced housing affordability, the finance theory suggests otherwise

– that through U+2 enforcement, renter willingness to pay is reduced through the preclusion of unrelated renter groups larger than three people.

College Students and Housing Insecurity

Recently in the United States, homelessness and housing insecurity affecting college students and young adults has become an increasingly visible and researched concern, though researchers lament that research in this field has field to keep pace with the growing phenomenon and its consequences (Clark et al 2017). Several recent papers have investigated rates of homelessness and housing insecurity, and how those conditions interact with student performance and rates of attrition (Tsui et al 2011; Silva et al 2015; Goldrick-Rab et al 2017; Kelchen et al 2017; Broton and Goldrick-Rab 2017; Hallet and Crutchfield 2017). Concurrently, research has also centered on initiatives directed at reducing incidence of housing insecurity and ameliorating its effects (Caton et al 2018; Crutchfield 2018; Hallet et al 2018). Despite an influx of research in recent years, the body of literature does not appear to have studied how zoning and occupancy standards can exacerbate college students' subjection to homelessness and housing insecurity.

Earlier research of household extension indicates, however, that U+2 enforcement might predispose college students to higher risk of housing insecurity and homelessness. While most extended households nationwide are composed of related persons in groups which would not violate U+2 (Koebel and Murray 1999), Corona's 2005 and 2009 inventories of violating households are constructed to not capture extended households comprised solely of families that would not violate U+2. In Fort Collins, because U+2 enforcement has made some household extension illegal in recent years, the city has removed students' ability to share the

costs of off-campus residence among groups larger than three students. Some studies of homelessness report that household extension is an observable antecedent of homelessness (McChesney 1987; Shinn et al 1991); therefore, U+2 enforcement may generate increased risk for homelessness among college-attending young adults, as well as the broader population of housing insecure renters in the city. Furthermore, if residents seeking opportunity to cost-share in groups ignore the ordinance and are caught in violation of occupancy standards, households are required to break up and downsize within a month to avoid fines.

Ultimately, irrespective of whether U+2 is found to reduce housing affordability in the city, its enforcement is out of alignment with research-based recommendations for policy more supportive of household extension. Domestically, Franck and Ahrentzen (1991) and Hemmens et al (1996) suggest loosening of restrictions on house sharing, while Jarvis (2013) reports on extensive benefits attributable to household sharing in Copenhagen amid a "deepening crisis in housing provision and access across Europe" (939). As demographic growth of house-sharing young adults continues to grow in Europe (Carlsson and Eriksson 2015; Schwanitz and Mulder 2015) and the United States (Mykyta 2012 in Clark et al 2017), occupancy restrictions such as U+2 will exacerbate disharmony between people's residential needs and local regulation.

Regulation and the Housing Market

If a population that extends households by necessity is denied the ability to do so, not only is cost-sharing unafforded to a vulnerable group that needs it, but that group might otherwise encounter a paucity of units with affordable rents. Research studying the causes and effects of local regulation that restricts land use or otherwise limits housing supply, as U+2 does, is plentiful. Much attention has been directed to this topic because "regulation appears to

be the single most important influence on the supply of homes" (Gyourko and Molloy 2014, 1). With respect to housing affordability, Gyourko and Molloy (2014) note that models consistently predict that regulation reduces the elasticity of housing prices and that the majority of research finds a strong positive relationship between regulation and house prices. The preponderance of reviewed studies find that locations with more regulation experience higher prices and less construction (Gyourko and Molloy 2014, 6, 42).

Theory suggesting a positive linkage between municipal, land use, or development regulation and the cost of developed land is long-established and generally uncontested. For example, see Pollakowski and Wachter (1990), who write, "[t]heory leads us to expect a positive effect of land-use restrictions on the price of developed land and a negative effect on the price of undeveloped land" (315), citing Muth and Wetzler (1976); Ohls, Weisburg, and White (1974); and Pogodzinski and Sass (1989). Furthermore, Gyourko and Molloy (2014), reviewing the studied relationship between regulation and housing supply, do not identify studies with contradictory findings.

The means by which restrictions can raise house prices are numerous. Quigley and Rosenthal (2005), reviewing 40 articles estimating the effect of regulation on housing prices, provide a table with 47 land use regulatory categories which may be linked with increased housing costs (74). Levine (1999) also produces a catalogue of land use policies and their effects on housing stock composition and price. Whether U+2 should be classified as a land use restriction could be argued; however, its status as a land use regulation is of minimal importance. Rather, the parallel application that many land use restrictions may share with U+2 is noteworthy.

Between the two papers, the authors identify a regulatory goal that is also identified as a pattern in Gyourko and Molloy (2014): through regulation, housing can be made to a higher quality and more expensive, increasing its profitability. About this, Gyourko and Molloy note that while it is a reasonable assumption that regulation yields increased housing costs, it must also be considered that high housing costs may lead to stricter regulation as homeowners seek to protect property values. Important here is the clear link between the latter relationship of regulation and housing costs, and the language with which the City of Fort Collins explains the impetus for enforcing U+2. On its U+2 page, the city cites a desire to "ensure health and safety of residents, and to help protect the quality and character of neighborhoods" (City of Fort Collins 2018). Taken at face value, these statements are innocuous. Literature studying the motivations for NIMBYism, however, which has drawn a link between such euphemistic language and efforts to optimize the commodity value of the home reflect also the influence homevoting may have exerted in bringing about U+2 enforcement in Fort Collins.

Because housing is a commodity and a financial technology through which people build wealth, when a something poses a threat – real or imagined – to property values, NIMBY forces mobilize (Koebel et al 2004; Pattillo 2013; Scally and Koenig 2013). Many amenities and disamenities beyond the physical characteristics of a home – such as school quality, nearby commercial establishments, crime rates, infrastructure maintenance, and zoning, to name a few – are capitalized through its price or rent. If, for example, the expansion of Colorado State University in the early 2000s brought many student renters into single-family residential neighborhoods, then the nuisance associated with student renters may have incited local homeowners to urge the city to begin enforcing U+2 in 2007. Disamenities associated with

college students, such as party-related noise and garbage, or additional cars parked on the street of once-quiet neighborhoods, might have caused a threat – real or perceived – to the "quality and character" of Fort Collins neighborhoods, such as Avery Park, which sees the greatest proportion of U+2-related activity in the city. Then, residents invested in protecting the commodity values of their homes (Conley and Gifford 2006; Pattillo 2013), might have been incented to work together politically to restrict undesired activity in their neighborhoods, according Fischel's (2001) *Homevoter Hypothesis.*

Conversely, the effects of U+2 enforcement may be more closely related to the findings of Morgan's (1984) study of density control on land prices. Morgan finds that when land is withdrawn from a developable inventory of land through growth management policies, the restricted supply leads to an increased bid rent, which does not run contradictorily to the logic expressed through studies above. But limiting density also renders land less valuable per acre as an input to new housing production. Perhaps by limiting residential density, U+2 enforcement might render houses for which there was once a rental demand to fill with unrelated persons, less valuable, as groups of smaller renters would have a lower willingness to pay for the same house.

Other Implications of Related Research

The researched connection between regulation and housing affordability – or the cost of renting developed space, more generally – tends to focus on regulation's role in increasing factor costs of development (Malpezzi 1996; Gyourko and Molloy 2014). U+2 does not directly interact with development costs, and if it has any influence on development, certainly does so to a vastly smaller degree than the city's growth management area (GMA); however, important

implications for the interaction of U+2 enforcement with housing affordability can still be gleaned from literature that studies regulation. The findings of studies comparable to mine indicate where my results might be more or less illustrative of the effects of U+2.

Important considerations can be found in Malpezzi (1996) and Pollakowski and Wachter (1990). Pollakowski and Wachter find that empirically examining land use control devices individually as though such devices operated independently may lead to underestimates of the impact of such regulations (315). Though U+2 is not necessarily a land use control, insofar as it is part of the regulatory framework affecting the spatial composition of people in Fort Collins, a study of its effect in isolation may yield results similar to those observed by Pollakowski and Wachter. Malpezzi (1996), which finds that regulation raises house prices more than it raises rents, concludes that regulation reduces homeownership based on correlations between prices and rents with homeownership rates. Ultimately, insofar as U+2 is a regulation, its enforcement should be expected to have rendered some negative impact on the affordability of housing in Fort Collins.

Estimation Strategy

The synthetic control method (SCM), popularized by Abadie, Diamond, and Hainmueller (2010), emerged from a thrust in the social sciences to bridge qualitative and quantitative empirical research methods in social sciences (Abadie et al 2015). The SCM meets this demand with the comparative case study, a long-established and often used research method, by allowing researchers to put “qualitative flesh on quantitative bones” – an expression in Tarrow

(1995). Employing a data-driven process to select weighted comparison units[3] in case studies, which creates a reliable control and enables quantitative inference in small-sample comparative studies, the SCM allows for more generalizable and robust comparison between the test subject and its control (Abadie et al 2010, 494; Abadie et al 2015, 495). Prior to the development of the SCM, empirical implementation of case studies was "plagued by inferential challenges and ambiguity about the choice of valid control groups" (Abadie et al 2010, 503). The logic of the SCM is that combining comparison units usually provides a better comparison for the treated unit than any single comparison unit could alone (Abadie et al 2010, 494). This method provides quantitative support to a comparative case study by creating a synthetic comparison unit that simulates what outcome the studied unit would have experienced were it not subject to treatment.

The SCM also addresses other significant shortcomings of case study methods that do not employ a synthetic control. For one, it is noted to reduce bias in observational studies (Abadie et al 2010). Because constructing the SCM does not require the researcher to assess postintervention outcomes of policy enforcement for the control unit, the researcher may design the study without knowing how study design will influence outputs, and therefore, conclusions (Abadie et al 2010, 494). Making decisions about study design without knowing how each decision affects outputs enables the researcher to operate in a fashion that fosters honesty and helps to prevent bias. This significantly promotes research transparency which cannot be comparably ensured in similar comparative observational methods, such as the

[3] In this study, the comparison units are cities, but in other research, the synthetic unit has been employed as a state, a region, a nation, etc.

difference-in-difference and least squares regression models (Rubin 2001; Abadie et al 2015). Furthermore, because the SCM uses data-driven procedures to develop the control unit, it reduces the researcher's discretion in its construction, instead producing the control through demonstrable and quantitative characteristics (Abadie et al 2010, 493-494). While it is difficult to secure an untreated control unit that accurately approximates the most relevant characteristics of the studied unit on its own, through the creation of a synthetic control, the comparison unit, constituted by a combination of donor units, provides a control unit that is not only less affected by bias, but which is also more reliable than any single unit might be in isolation.

The SCM also provides improved transparency and safeguards against extrapolation, which also make the method preferable to traditional regression methods. First, by producing a weighted average of available control units and predictor variables, the SCM displays the relative contribution of all constitutive factors to the synthetic control. The output also quantifies the difference between the studied unit and the synthetic unit, illustrating how postintervention outcomes are affected by treatment. Additionally, because predictor variables must all be positive and together sum to one, the synthetic control does not allow for the kind of extrapolation biases large-sample regressions are subject to. (Abadie et al 2010, 493-496) This supports a more precise comparison between the treated unit and the comparison unit than other common comparative case study methodologies (Abadie et al 2015; King and Zeng 2006). Abadie et al (2015) show that while regression estimators can be expressed as a weighted average of compared units with weights that similarly sum to one, regression weights are not restricted to lie between zero and one, which allows extrapolation. In sum, these

features of the SCM enable the researcher to employ reliable and transparent qualitative and quantitative techniques to contrast the treated unit with the control.

Constructing the Synthetic Control

Though each application of the SCM varies, I generally follow the methodology outlined in Abadie et al (2010). Much of this process is automated in *Synth*, the open-source software package made available by the authors.[4] First, one must identify potential donor units – the donor pool – which are synthesized to construct the control unit. Because the SCM compares an approximated counterfactual composed of a fixed combination of donor units to the treated unit, donor units must have predictor values (explained below) that roughly reflect the studied unit prior to treatment (McClelland and Gault 2017; Abadie et al. 2010). Importantly, donor units also must not be treated by the same or similar policies for any year relevant to the study (Abadie et al 2010; Abadie et al 2014).

In their study of an anti-tobacco law enacted in California, which popularized the SCM, Abadie et al (2010) note that they removed 4 states from the donor pool because these states implemented similar policies during the studied period (498-499). Therefore, I do not include in the donor pool any cities which have enacted similar residential occupancy restrictions during the studied period. To avoid overfitting – when idiosyncratic variations of a large sample of unaffected units artificially match the studied unit – Abadie et al (2015) also recommend that researchers reduce the size of the donor pool by only selecting donor units which are similar to

[4] Companion software developed by the authors (Synth package for MATLAB, R, and Stata) is available at the authors' webpages. The R source package used in this study can be accessed at https://www.jstatsoft.org/article/view/v042i13

the treated unit (500). Not only does this reduce overfitting but it also facilitates the satisfaction of Hahn and Shi's (2017) recommendation to employ a large number of predictor variables compared to the number donor units. To construct the synthetic control, I use a donor pool of 15 cities that exhibited similar population growth dynamics to Fort Collins between the 1990 and 2000 Censuses (Corona Insights 2005, 85).

Next, one must identify and weight predictor variables for the outcome variable. Unit-level panel data is collected for both pre- and post-treatment periods. Good predictor variables affect outcomes in all units before and after treatment (McClelland and Gault 2017, 7), and predictor variables should have a stable relationship with the outcome variable to ensure their use is appropriate and not random. As Hahn and Shi (2017) identify, a large number of predictor variables relative to the number of donor units improves the choice of weights that might be assigned to each donor, rendering the synthetic counterfactual a better predictor of nonintervention outcomes. Once it is determined that selected predictor variables exercise some influence on the studied outcome, the pretreatment time frame is selected. A longer pretreatment range is best, and notably, a predictor variable doesn't have to be rejected if data is unavailable for some of the pretreatment years (McClelland and Gault 2017, 7). I employ a set of 13 predictor variables, one of which is a lagged outcome variable.

Several authors argue that lagged values of the outcome variable make an important predictor of the outcome variable; however, there is some degree of disagreement for the appropriate use of this variable. Athey and Imbens (2006), for example, argue that if one includes the lagged outcome variable for some pre-treatment year, other covariates rarely matter. This suggests that use of the outcome variable for all pre-treatment years is not a

reliable means for producing the counterfactual unit. Kaul et al (2016) agree that this often eliminates the effect of other predictors and they add that removing the effect of other predictor variables may skew the outcome variable for the synthetic unit in post-treatment periods. To construct the synthetic control, I adhere to the findings of Abadie et al (2010) and Ferman et al (2016), who indicate that several sets of lagged outcome variables should be tested to create a more reliable post-treatment synthetic unit.

The third step in the SCM requires that one chooses a method for selecting predictor weights. These unit-level weights determine how the synthetic control is formed. According to Abadie et al (2010), several procedures for selecting weights of donor cities can produce a valid analysis, so long as donor unit weights are non-negative and their sum is equal to one. The first method for selecting donor weights is by the subjective choice of the researcher, which bypasses econometric procedures. Abadie et al (2010) minimize the outcome variable's mean-squared prediction error (MSPE) in pre-treatment years by choosing weights from all possible matrices. The MSPE in Abadie et al (2014) refers to difference between the "average of the squared discrepancies between outcome variable results in the treated unit and in its synthetic counterpart during the studied period (501). Abadie et al (2015), seeking to reduce overfitting, employ a cross-validation method that divides the pretreatment period into two sub-periods. The first is a training period that is used to reduce the MSPE of the second, validation period (Abadie et al 2015, 501-502). Because the data available for this study does not produce a pretreatment period long enough to subdivide into a training and validation period, I resort to the default function of *Synth,* because it produces weights which minimize the MSPE and provides the optimal fit, which is recommended in Abadie et al (2010).

Once this process is completed, the pre-treatment period goodness of fit for the control unit is assessed. This is an output of the *Synth* software package that runs the SCM. The primary output of *Synth* is a pre- and post-treatment vector for the studied and synthetic units' outcome variable. Goodness of fit – how closely the two paths follow each other in pre-treatment period – can first be checked visually, but is quantitatively checked by calculating the MSPE. If fit is poor, McClelland and Gault (2017) recommend one use a model that employs all possible outcome lags to test whether the control unit can match the treated unit sufficiently. If the fit is not good, the authors suggest the SCM cannot be used as it is built, because no model provides a good fit. With this, the authors note that using all possible lags, which can create a good fit in the pre-treatment period, may bias the post-treatment outcome of the synthetic unit (8-10).

The next step in determining goodness of fit requires that donor unit weights are reviewed to judge similarities between donors and the treated unit. Here, outcomes in donors need not display a similar average to the treated unit, and can instead display similar trends. Finally, predictor weights are reviewed to investigate how powerfully different predictor variables explain the outcome (Abadie et al 2010; McClelland and Gault 2017).

Data and Sample

I use annual city- and MSA-level data for the period 1996 to 2017. Although the ecological unit of study is the city, the metropolitan statistical area (MSA) is occasionally used, as data for important predictor variables is often not kept annually for cities. To ensure that MSAs still provide a good fit, sensitivity tests are performed, and no donor cities belong to the highly proximal MSAs that might be part of the same real estate submarket, so as to ensure

individuality among donor units. U+2 enforcement began on January 1, 2007, providing a preintervention period of 11 years. Because Zillow's home value index (ZHVI) – the best publicly available estimate of housing cost appropriate to this study – is only available as far back as 1996, the preintervention period is only 11 years. In contrast, other studies (Abadie and Gardeazabal (2003); Abadie et al (2010); Abadie et al (2015)) tend to employ preintervention periods in the range of 30 to 40 years. The sample period terminates in 2017, creating a span of prediction as roughly long as the preintervention period.

Synthetic Fort Collins is constructed as a weighted average of cities in the 15-city donor pool: Greensboro (NC), Provo (UT), Sioux Falls (SD), Salem (OR), Winston-Salem (NC), Eugene (OR), Durham (NC), Fort Wayne (IN), Joliet (IL), Lincoln (NE), Raleigh (NC), Lexington-Fayette (KY), Mesquite (TX), Columbia (SC), and Lakewood (CO). A comprehensive list of all the predictor variables used to construct the synthetic city is provided later, in Table 3. The appendix also lists all variables used, how they were calculated (where appropriate), as well as their sources. To capture the characteristics of Fort Collins that contribute to the cost of housing in the city prior to U+2 enforcement, and therefore, construct the synthetic city, I use a reasonable set of variables that inform the cost of housing. These predictor variables include population and the growth rate, the city's economic constitution (expressed as the location quotients for the five industries with the highest location quotients in Fort Collins in 2007), homeownership rate, building permits, and total housing units. These variables are collected only for cities, where available; however, where city-specific data is not tracked annually, I use MSA-level data. The ZHVI is the outcome variable. Though it is neither a pure reflection of sale

prices nor of rental costs, the ZHVI is the best publicly available proxy for either of these predictors.

The ZHVI for each year is calculated as the average of all monthly ZHVIs from a given year in a given city, as Zillow produces a dataset which charts the ZHVI for every city on a monthly basis. It is important to note that ZHVI figures for 2007, for example, do not reflect the home value figure for the beginning of that point, but rather for the average of all months in 2007. This is an important distinction to make before examining the results of the synthetic control method below.

Results: The Impact of U+2 Enforcement on Housing Affordability

To evaluate the effect of U+2 enforcement on housing costs in Fort Collins, the central focus of this comparative case study is how the cost of housing would have evolved in Fort Collins after 2007 in the absence of U+2 enforcement. Using the approach outlined in the estimation section, I construct a synthetic Fort Collins with weights of donor units and predictor variables chosen so that the synthetic city best imitates the values of predictors of Zillow's home value index (ZHVI) in Fort Collins during the preintervention period, then estimates the trajectory of Fort Collins' home value after intervention.

While I do not subdivide the preintervention period into a training and validation period, I use a function in *Synth* to minimize the sum of squared residuals between Fort Collins and its synthetic counterfactual between 2002 and 2006. This works similarly to the preintervention training and validation that Abadie et al (2015) employ, helping the synthetic Fort Collins better match the actual outcome during preintervention; however, it does not require as long of a pretreatment period to produce useful outputs. I choose these years as they represent the

latter half of the preintervention period and prepare the program to accurately represent the divergence between Fort Collins and its synthetic control in 2007. Once I run *Synth*, the program produces three tables that identify the makeup of the synthetic city by predictor variables used and their weights, constitutive cities of the synthetic and their weights, and predictor means, which I explain below. It also produces two figures, which plot the trends in ZHVI in Fort Collins and its synthetic, as well as the gaps between those two vectors.

The synthetic Fort Collins produced through *Synth* is a combination of states from the donor pool that most closely resemble Fort Collins' preintervention predictor values. The results are presented in Table 1, which compares the preintervention characteristics of Fort Collins, its synthetic, and the mean of the entire donor pool. The table shows that the average of the 15 control cities does not provide a suitable control group for California. In particular, prior to U+2 enforcement, change in ZHVI and population growth rates of Fort Collins were poorly matched by the average, but replicated through unit-weighting performed by Synth. Additionally, lagged ZHVI data for the years 2002, 2004, and 2006 are matched very closely by the synthetic but quite poorly by the sample mean.

Table 1. U+2 enforcement predictor means			
	California		
Variables	Real	Synthetic	Average of Donor Pool
Change in ZHVI	0.032	0.032	0.047
Permits	2883.6	17910.094	11456.4
Homeownership Rate	0.55	0.619	0.576
Population	272.244	2069.716	1442.083
Pop. Growth	0.015	0.015	0.017
Housing Units	53964	61472.838	73523.633
NAICS 23 LQ	1.718	1.345	1.162
NAICS 72 LQ	1.756	1.128	1.211
NAICS 44-45 LQ	1.318	0.995	1.241
NAICS 42 LQ	0.465	0.935	0.79
NAICS 54 LQ	0.995	1.223	1.086
ZHVI lag 2002	202700	202135.755	129219.444
ZHVI lag 2004	215066.667	214816.17	141301.667
ZHVI lag 2006	202700	202135.755	129219.444

Table 1 also shows that for some predictor variables, the synthetic city does not produce a better fit to Fort Collins than the sample mean. *Synth* addresses this problem by weighting the contribution of predictor variables as well as donor cities, and excluding predictors whose data fall far outside the convex hull of Fort Collins' preintervention dataset. Table 2 shows the weights *Synth* assigns to cities in the donor pool, and Table 3 shows the weights *Synth* assigns to predictor variables to construct the synthetic Fort Collins. Together, these three tables underscore an important feature of the SCM: it is only reliable if one can demonstrate affinity between the treated unit (Fort Collins) and its synthetic counterfactual, and it forces the exclusion of counterfactuals with extreme predictor data.

Table 2. City weights in synthetic Fort Collins

City	Weight	City	Weight
Greensboro	0	Joliet	0
Provo	0	Lincoln	0
Sioux Falls	0.1	Raleigh	0.003
Salem	0.021	Lexington-Fayette	0
Winston-Salem	0.001	Mesquite	0
Eugene	0	Columbia	0
Durham	0	Lakewood	0.875
Fort Wayne	0		

Table 2 displays the weights of each donor city in synthetic Fort Collins. The weights displayed in the table indicate that from the provided set of predictors, ZHVI in Fort Collins is best recreated through a combination of Lakewood, Salem, Sioux Falls, Raleigh, and Winston-Salem. All other cities from the donor pool are assigned zero weights and therefore, do not contribute to the synthetic counterfactual. Similarly, Table 3 shows the weights of predictor variables. Lagged ZHVIs from donor units unsurprisingly constitute the highest proportion of variable weights. Year-over-year change in ZHVI, population growth, location quotients relative to the construction industry and professional, scientific, and technical services comprise the remaining predictor variables.

Table 3. Variable weights in synthetic Fort Collins

Variable	Variable Weights
Change in ZHVI	0.327
Permits	0
Homeownership Rate	0
Population	0
Pop. Growth	0.166
Housing Units	0
NAICS 23 LQ	0.002
NAICS 72 LQ	0
NAICS 44-45 LQ	0
NAICS 42 LQ	0
NAICS 54 LQ	0.045
ZHVI lag 2002	0.144
ZHVI lag 2004	0.171
ZHVI lag 2006	0.144

Figure 1 displays the ZHVI for Fort Collins and the synthetic counterfactual during the period 1996 to 2017. The vertical line plotted between 2005 and 2010 indicates 2007, the year when U+2 enforcement began. The plotted lines show that ZHVI in the synthetic city very closely track the trajectory of the outcome variable during the preintervention period, but then the two diverge shortly after 2007. Consider the degree to which predictor values from Table 3

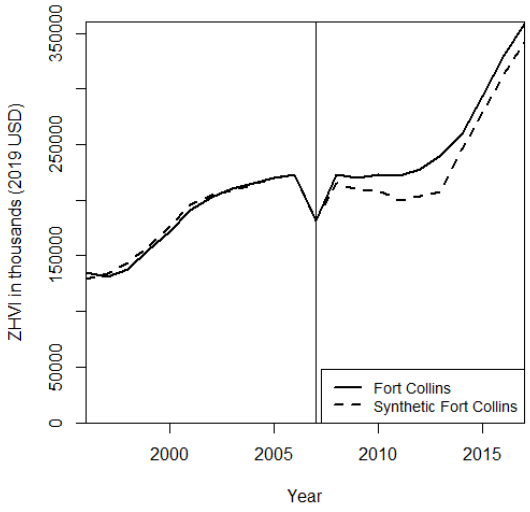

Figure 1. Trends in ZHVI: Fort Collins vs Synthetic Fort Collins

in the synthetic city match those in Table 1. Together, these outputs suggest that the synthetic counterfactual provides a useful approximation of the ZHVI actually experienced in Fort Collins during the preintervention period; therefore, it is sensible to deduce that the synthetic provides a useful approximation for what ZHVI would have been in Fort Collins in the absence of U+2

Synth enables me to estimate the effect of U+2 enforcement on ZHVI (cost of housing). This is the difference between ZHVI and its synthetic counterpart. An exaggerated downward then upward spike occurs between 2006 and 2008, with the trough occurring as the 2007 ZHVI, it is unreasonable to assume that this is spike has anything to do with U+2 enforcement, as this pattern is reflected in placebo tests conducted for all contributing donor units. Additionally, the gap in ZHVI between the does not appear until 2008. Recall that the ZHVI for any particular year is the average of the ZHVI during all months of that year, and so, it is not a reflection of the start of that year, but an average of monthly ZHVIs at the end of the year.

Notice that the gap in ZHVI is not immediately identifiable, but it becomes pronounced beginning in the year 2008. This pattern is likely observed because, if U+2 enforcement was indeed perceived to eliminate a disamenity among homeowners in Fort Collins, the ensuing increase of home value would only materialize as enforcements of U+2 mitigated the disamenities the ordinance targets (cohabiters, usually renting college students) during the

course of the first year of intervention. Such a lag should be expected, as home sales and rent adjustments would require time and some iteration to capitalize as an increased ZHVI. Figure 2 plots the estimated impact of U+2 enforcement on ZHVI in Fort Collins by displaying the gaps between the actual ZHVI of Fort Collins and that of the synthetic city between 1996 and 2017.

Figure 2 again displays that preintervention tracking of ZHVI in Fort Collins and the synthetic city are quite close. Then, after U+2 enforcement began in 2007, home values in Fort Collins increased significantly in comparison to the synthetic city. Visually, the effect appears significant, but it is important to note that the gap never exceeds $4,000 in home value. Such a gap is not immaterial, for it represents an increase in average home value ranging between 1 and 2 percent citywide in the first six years following 2007 even though U+2 enforcement is concentrated in Avery Park (a one-half square mile area) and CSU-proximate areas (Douglas 2016); however, it is modest. An important additional inquiry not included in this study would assess how U+2 enforcement affected the cost of housing in parts of the city comparatively in parts of the city where U+2 enforcement is concentrated, as opposed to the city on a whole.

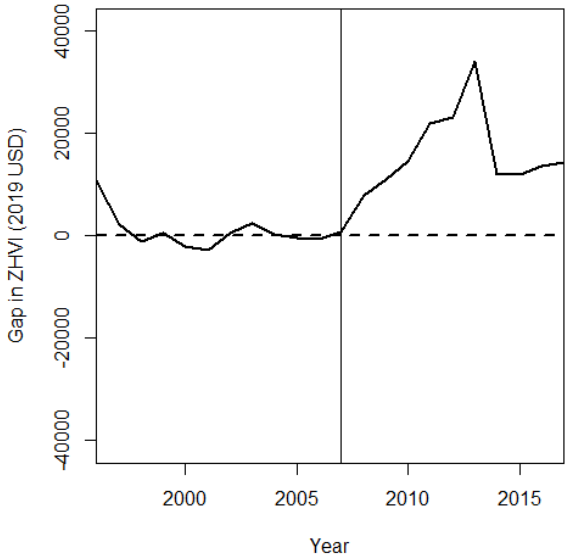

Figure 2. Gaps in ZHVI: Fort Collins vs Synthetic Fort Collins

Placebo and Robustness Tests

To assess the reliability of my results, I perform additional placebo and robustness tests. These tests suggest that the predictor variables and donor cities I use to estimate a synthetic Fort Collins are appropriate for the subject, though they produce outcomes that are sensitive to changes in the model. When testing different sets of lagged ZHVIs as predictor variables, the distribution and weighting of other predictors and donor cities usually changed significantly. With some sets of lagged ZHVIs, I could reduce the preintervention MSPE significantly, producing what should be expected to be a more robust synthetic control; however, doing so would usually render the model overdependent on lagged ZHVIs and sometimes on some other variable, which Athey and Imbens (2006) warn against. I settle on a final grouping of lagged ZHVIs for 2002, 2004, and 2006, because this set of lagged ZHVIs produces a synthetic city built from a diversity of variables and donor cities, and weights. Despite some sensitivity in the model, it produced a consistent prediction in every iteration: my synthetic city always reflected a lower prediction of ZHVI in postintervention periods than the actual ZHVI, indicating that U+2 enforcement did modestly increase the cost of housing across Fort Collins.

The brevity of the preintervention period caused by lack of good data creates a synthetic city which may is an imperfect estimator of postintervention ZHVI outcomes in the synthetic city. Furthermore, data during the preintervention period for important predictors like homeownership rates, housing unit counts, and demographic breakdown are sparsely available through open access public datasets. It follows, then, that the data used to inform the synthetic city leaves quality to be desired. Another drawback to the data used is related to the

geographic scope of some datasets. For many predictor variables – homeownership rates, total housing units, key industry location quotients – I had to rely on MSA-level data. As MSAs are frequently much larger than the cities I study which compose them, and because different cities differently contribute to the trends expressed in MSA data, these predictor variables are imperfect estimates of such trends in my geographies of interest.

Placebo Tests

I conduct placebo studies wherein the treatment of interest (U+2 enforcement) is reassigned to all other cities in the donor pool. Usually, placebo tests are assigned at varied preintervention time-periods, but given the transience of my preintervention data series, this test is unreliable. Using the same data panel, I assign for each of the 15 donor cities to be run as the test subject to estimate the chance that my results could be driven by chance, rather than statistical significance. If placebo studies show that the magnitude of the gap for ZHVI in Fort Collins and its synthetic is frequently reproduced, then I interpret that the analysis does not provide significant evidence that U+2 influenced the cost of housing. Conversely, if the placebo studies show that the gap in ZHVI is unusually large in Fort Collins compared to its donor pool, then there is evidence that U+2 indeed negatively affected housing affordability in Fort Collins.

I iteratively apply the identical SCM to the 15 donor cities to estimate how U+2 enforcement affected other cities in the donor pool. In each placebo test, I run *Synth* as if one of the cities in the donor pool passed a similar occupancy restriction ordinance in 2007. For each placebo test, Fort Collins is moved into the donor pool. I then plot in Figure 3 the estimated gaps for the donor pool cities where U+2 was not enforced.

Figure 3. Gaps in ZHVI for all placebo tests

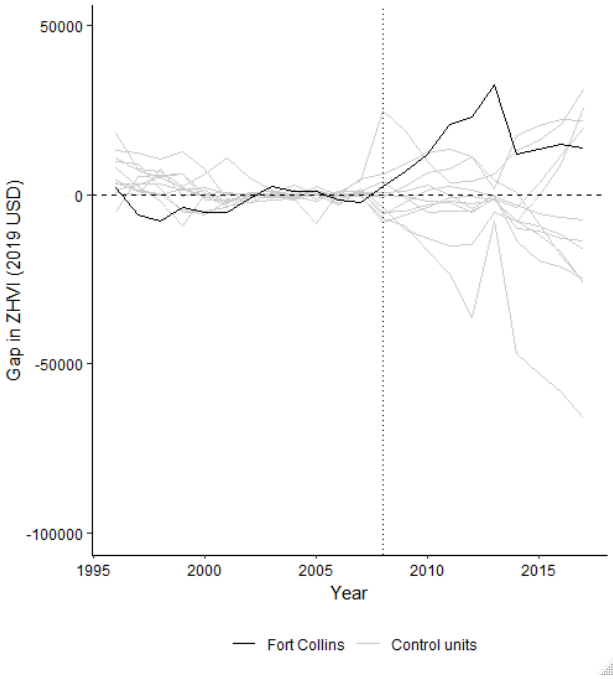

Figure 3 shows the results for all placebo tests. The gray lines represent the ZHVI gap for all donor cities, while the black line is a reproduction of that for Fort Collins. The gray lines, then, exhibit that the difference in ZHVI between donor pool cities and their synthetic counterparts. This figure portrays mixed results. For much of the postintervention period, Fort Collins displays a gap that is indeed unusually large. After 2013, though, the magnitude of Fort Collins' gap comparatively shrinks, and significantly so. Numerous placebos exhibit gaps of greater margin than Fort Collins after 2013.

Robustness Tests

As Figures 3 and 4 indicate, however, the MSPE in Fort Collins (the average of the squared discrepancies between ZHVI in Fort Collins and the synthetic counterpart) is small before intervention and much larger afterwards. This produces a high post-/pre-intervention MSPE ratio, which is the difference between the observed outcome of a unit and its synthetic control, before and after intervention. A higher ratio indicates a small preintervention error and large postintervention MSPE, or a large difference between the city and its synthetic control after the intervention. By calculating this ratio for all placebos, I examine how likely the result obtained for Fort Collins could have occurred by chance in the absence of U+2 enforcement. This effectively produces a p-value, which is reached by calculating the fraction of such effects

greater than or equal to what is observed in Fort Collins following U+2 enforcement (Abadie et al 2015). This p-value can also be interpreted as the probability of obtaining an estimate as large as that obtained for Fort Collins in this study when the intervention is randomly assigned to another donor city. Figure 4 presents the results of the MSPE post-/pre-intervention ratio.

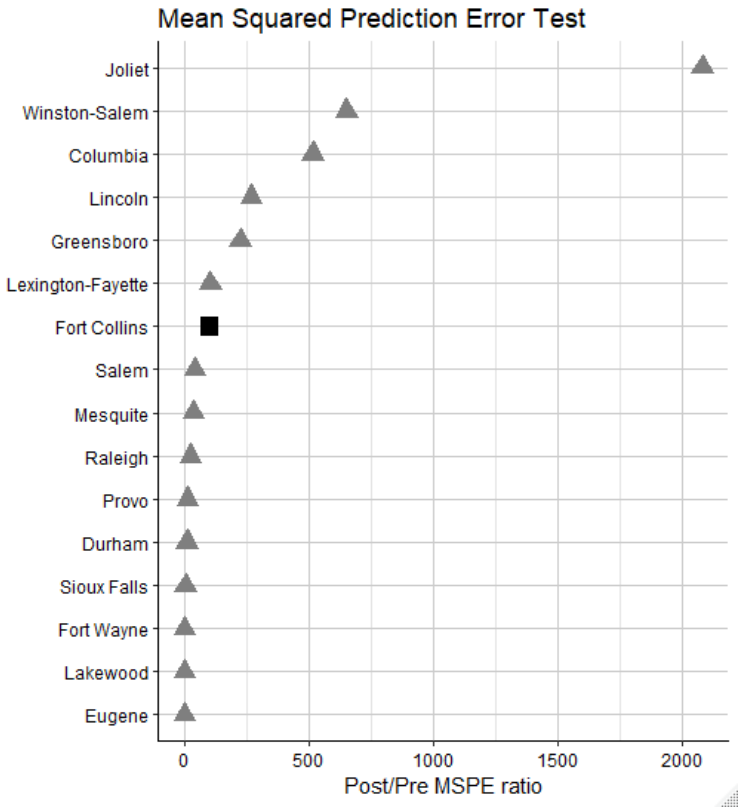

Figure 4. Post-/Preintervention MSPE ratios for Fort Collins and donor cities

Fort Collins' p-value for this SCM is 0.4375, a value too low to warrant statistical significance. Importantly, however, of all donor cities, the only donor city with a higher MSPE ratio than Fort Collins is Winston-Salem, and Winston-Salem is weighted as 0.1 percent of the synthetic city. A donor unit with a higher MSPE ratio than the test unit is not considered an appropriate donor (Abadie et al 2015). Though Winston-Salem only constitutes a small fraction of the synthetic city, it is a poor donor city no less. In sum, my placebo and robustness tests

indicate that the panel of predictor variables and cities appropriately fit Fort Collins, though the panel does not produce outcomes of statistical significance.

Limitations of Study

Although the outputs of *Synth* indicate that the set of predictor variables and the donor pool appropriately constitute a synthetic city which can imitate the cost of living in Fort Collins, a few limitations to this study preclude the possibility of reaching statistically significant outputs. The limitations of this study can be categorized as data issues and inferential difficulties.

Most of the issues related to data used in this study are outlined above, but the limitation of the preintervention period's brevity warrants more discussion. Abadie et al (2010; 2015) note that application of the SCM requires "a sizeable number of preintervention periods" and the authors do not recommend using the method if the preintervention period is short (2015, 500). This is because much of the credibility of the synthetic control is derived from the span of preintervention time modeled and how closely the synthetic unit can track preintervention outcomes in the treated unit over that time. Not only does this exhibit robustness of fit and optimize the post-/pre-intervention MSPE ratio, but it also ensures that prediction of postintervention outcomes is more reliable. With an 11-year preintervention period using data collected annually, this study is unable to meet the authors' recommendation. Although the synthetic Fort Collins tracks the actual city's outcome variable fairly well, especially in the latter half of the preintervention period, the period is too short to produce a very reliable postintervention estimate.

The inferential methods used to produce the study's p-value may also complicate the SCM's ability to produce results with great statistical confidence. Recall that the p-value (0.4375) was reached by calculating the number of placebo outcomes that expressed a gap in real and synthetic ZHVIs of equal or greater magnitude than that produced between the real and synthetic Fort Collins. The premise of this p-value test is that my "confidence that a sizable synthetic control estimate reflects the effect of the intervention would disappear if similar or larger estimates arose when the intervention is artificially reassigned to units not directly exposed to the intervention" (Abadie et al 2015, 500). The authors later note that the inferential exercise which produces the p-value in this study is "restricted to the question of whether or not the estimated effect of the actual intervention is large relative to the distribution of placebo effects" (Abadie et al 2015, 500). Accepting the outputs of the SCM above despite its flaws, which identifies an influence never in excess of $4,000 in change on home value (equating to a one to two percent increase in home value during the postintervention period), it becomes clear that using the SCM to estimate the influence U+2 enforcement on home value in Fort Collins may be very tricky. First, the tracking of the synthetic to the real ZHVI in the pretreatment period must fit extremely well in order for the model to express with any confidence that home values are indeed influenced by enforcement of the ordinance. Additionally, as Abadie et al (2015) note, donor units that may have suffered large idiosyncratic shocks to local home values between 1996 and 2017 would need to be excluded if those shocks would not have affected the treated unit in the absence of treatment. Ensuring that no donor cities would have experienced such an idiosyncratic shock with influence of such a small magnitude would be nearly impossible and would require extensive

historical market analysis of all potential donor units. Together, these limitations render the SCM a difficult approach to measuring the subtle effect of U+2 enforcement on housing costs in Fort Collins. Instead, an additional study identified in the results section – one which compares the effect of U+2 on housing costs in areas of concentrated enforcement – might better illustrate the degree to which this ordinance has influenced the cost of housing in particular subsections of Fort Collins.

Conclusion

Despite its sensitivity, the SCM provides results useful for estimating its impact on the cohabiting population U+2 targets. Further, these results accurately reflect what theory addressing the relationship of regulation and housing price suggests: insofar as U+2 is a regulation of the housing supply in Fort Collins, its enforcement should yield increased housing costs. Furthermore, the magnitude of effect rendered by U+2 may be understated, as Pollakowski and Wachter (1990) suggest that empirical studies of land use controls individually as though such implements function independently may produce underestimates of the outcomes of such regulations. While not a land use control, U+2 is part of the regulatory framework dictating the organization of residents in Fort Collins.

Using Costar's cap rates from the studied period, 2007 to 2017, the change in cost of rented housing can be extrapolated from the percent change in ZHVVI. Cap rates ranged between 4.5 and 6 percent during the studied period, and the years when U+2 most dramatically increased housing costs in Fort Collins, it did so by a magnitude of roughly $3,000 citywide. Using these figures, assuming that home price is the present value of discounted possible cash flows, it can be estimated that annual rents citywide would have increased by a

value between $135 and $180. Consider that such rents, then, would be split among smaller groups of renters due to U+2 enforcement. The impact of U+2 enforcement on housing costs of previously violating renters is magnified as smaller groups of people would ostensibly be forced to share rising rents.

I find that U+2 enforcement modestly increased the cost of housing citywide, by a range of one to two percent in excess of estimated nonintervention prices. This increase in home value, capitalized into annual gross rent can be estimated as a range of $135 to $180 in increased renting cost. While modest, this increase in rent is not immaterial: the cost of housing, to be split among smaller groups of people, negatively influences the affordability of housing primarily for a population that faces insecurity of basic needs at a rate much higher than what is experienced on average across the country. U+2 enforcement is a detriment to the affordability of housing in Fort Collins; it is a detriment particularly to a qualitatively more vulnerable population.

Appendix

All predictor variables, their sources, methods of calculation, and peculiarities are as follows:

- The Zillow Home Value Index (ZHVI) and Change in ZHVI variables are both available through Zillow's research pages. Change in ZHVI is calculated year-over year.
- Homeownership rates are sourced from the US Census Bureau and the American Community Survey. These data are only available nationally at the MSA level.
- Total Housing Units are sourced from the US Census Bureau and the American Community Survey. These data are only available nationally at the MSA level.
- Building permits are sourced from the US Census Bureau and accessed through the St. Louis Federal Reserve website. These data are only available at the MSA level.
- Population and Population Growth Rate are sourced from the US Census Bureau and the ACS.
- Location quotients for the five industries in 2007 (2-digit NAICS codes) with the highest local concentration in Fort Collins compared to the national concentration, are sourced from the US Census Bureau's county business patterns. Instead of using national location quotients in constructing the control, I use a dataset location quotient so that the denominator of the location quotient (the "global" concentration of an industry) is the sum of employees working in some industry in the 16 MSAs that construct the data panel divided by the sum of all employees in all 16 MSAs for that year. The five 2-digit industry NAICS codes I use are: 23, 72, 42, 54, and a combination of industry codes 44 and 45.

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